XXXII. *An Account of ſome Experiments made with an Air-pump on Mr.* Smeaton's *Principle; together with ſome Experiments with a common Air-pump. By Mr.* Edward Nairne, *F. R. S.*

Read June 12, 1777.

AS the following experiments were made principally to try the performance of Mr. SMEATON's pear-gage, it may be proper to deſcribe it, which I ſhall do in his own words, taken from the Phil. Tranſ. for the years 1751 and 1752, vol. XLVII. p. 420.

" I have found," ſays Mr. SMEATON, " the gages that " have been hitherto made uſe of, for meaſuring the " expanſion of the air, very unfit to determine in an experiment of ſo much nicety; I have therefore contrived one of a different ſort, which meaſures the expanſion with certainty to much leſs than the 1000th " part of the whole. It conſiſts of a bulb of glaſs, ſomething in the ſhape of a pear, and ſufficient to hold

" about half a pound of quickſilver: it is open at one " end, and at the other is a tube, hermetically cloſed " at the top. By the help of a nice pair of ſcales I found " what proportion of weight a column of quickſilver, of " a certain length, contained in the tube, bore to that " which filled the whole veſſel: by theſe means I was " enabled to mark diviſions upon the tube anſwering to " a 1000th part of the whole capacity; which being " about one-tenth of an inch each, may, by eſtimation, " be eaſily ſub-divided into ſmaller parts. This gage, " during the exhauſting of the receiver, is ſuſpended " therein by a ſlip wire. When the pump is worked as " much as ſhall be thought neceſſary, the gage is puſhed " down till the open end is immerged in a ciſtern of " quickſilver placed underneath; the air being then let " in, the quickſilver will be driven into the gage till the " air remaining in it becomes of the ſame denſity with " the external, and as the air always takes the higheſt " place, the tube being uppermoſt, the expanſion will be " determined by the number of diviſions occupied by " the air at the top.

" The degree to which I have been able to rarify the " air in an experiment, has generally been about 1000 " times, when the pump is put clean together; but the

" moiſture that adheres to the inſide of the barrel as " well as other internal parts, upon letting in the air, is " in the ſame ſucceeding trials worked together with the " oil, which ſoon renders it ſo clammy as to obſtruct " the actions of the pump upon a fluid ſo ſubtil as the " air when ſo much expanded; but in this caſe it ſeldom " fails to act upon the air in the receiver till it is ex- " panded 500 times." Thus far Mr. SMEATON's account.

The pump with which the chief of the following experiments were made, had the leather of its piſton ſoaked in oil and tallow (and oil in the barrel) and every precaution was taken that no water ſhould get into the working parts of the pump, except what might ariſe in vapour from the ſubſtances which were under the receiver.

EXPERIMENT I.

Having provided a pear-gage agreeable to Mr. SMEATON's deſcription, on which the ſpace of a 4000th part of the whole capacity was two-tenths of an inch; this gage, together with a glaſs cup which ſerved as a ciſtern to hold the quickſilver for it, was put under a receiver; which receiver was placed on a leather ſoaked in oil and tallow, on the plate of the pump.

I muſt obſerve here, that the foot of the glaſs cup, which held the quickſilver for the pear-gage, being broken off by an accident, another foot, made of a piece of box-wood, was cemented to it. The reaſon of mentioning this circumſtance, which may ſeem trifling, will appear in the ſixth experiment. The pump was then worked, and the pear-gage was puſhed down till its open end was immerſed in the quickſilver in the cup: the air being then let into the receiver, it forced the quickſilver into the gage till it was filled within a 4000th part of the whole, ſo that by this gage the pump appeared to have expanded the air 4000 times. To what it might be owing that this pump, which was on Mr. SMEATON's principle, ſhould expand the air 4000 times inſtead of only 1000 as Mr. SMEATON's, I could not even ſurmiſe.

Having the pleaſure of knowing Mr. SMEATON, and being well acquainted with his great abilities in practice as well as theory, I could not imagine that this apparent ſuperiority could proceed from our having executed the various parts of the pump in a more perfect manner than he had done. I therefore determined, for greater certainty, to ſee if the fault might not be in the gages; and for that purpoſe I repeated the experiment with the ſyphon-gage, and both the long and ſhort barometer-gages, and found that the ſeveral degrees of exhauſtion indicated by theſe, were very different from that which the pear-gage had indicated: no concluſion, therefore, could be drawn from this experiment.

I determined next to compare again this pear-gage with the long and ſhort barometer-gages with all the accuracy I was maſter of: and firſt, leſt the tubes of theſe barometer-gages might not be perfectly clean and free from moiſture, I had ſome tubes made at the glaſs houſe; and as ſoon as they were brought home, which was within an hour after they were made, two of them, which were of the ſame ſize, were filled with diſtilled quickſilver; and then the quickſilver was carefully boiled in the tubes the whole length, which was about thirty-ſix inches: I then cut off about ſix inches from the ſealed end of one of the tubes, and took care to keep it perfectly full of the boiled quickſilver; it was then inverted into a glaſs ciſtern containing boiled quickſilver; and a piece of very thin ivory, about half an inch in length, with diviſions on its edge, was put over the tube, ſo as to float on the ſurface of the quickſilver in the ciſtern, by which means the difference of the two ſurfaces could be ſeen to a great nicety. This kind of gage is called the ſhort barometer-gage.

The other tube, which was cut off to thirty-three inches, being perfectly full of the boiled quickſilver, was alſo carefully inverted into a glaſs ciſtern containing boiled quickſilver, to ſuch a depth that from the ſurface of the quickſilver in the ciſtern to the top of the tube was twenty-nine inches; this had likewiſe a piece of ivory, with diviſions on it, put over the tube, ſo as to float on the ſurface of the quickſilver in the ciſtern, in the ſame manner as the other.

This long tube and the ſhort barometer-gage being put both of them at the ſame time under the receiver, which was carefully cemented to the plate of the pump; the pump was then worked for ten minutes, and the ſurface of the quickſilver in both the tubes came down very nearly to within one-twentieth or five hundredth parts of an inch of the ſurface of the quickſilver in their reſpective ciſterns.

The air was then let in, and the receiver being taken from the pump, the long tube was raiſed up ſo far in the ciſtern as to let the quickſilver come down from the top of the tube, ſo that it now became a common barometer, and its height from the ſurface of the quickſilver in the ciſtern meaſured thirty inches; which agreed exactly with an open ciſtern-barometer I had in the room. The quickſilver in the tube of this barometer was alſo boiled, and the meaſure the ſame.

The quickſilver was then emptied out of this long tube, and the ſealed end being cut off, it was then cemented to a piece of braſs, by which means it was ſcrewed to the air-pump; and the lower end being immerſed in a cup of boiled quickſilver, it then made that kind of gage where the air is taken from the top of the tube, and which is called the long barometer-gage. This gage being fixed to the pump, and the ſhort barometer-gage put on the plate of the pump under a receiver, the receiver was cemented to the plate of the pump, and the pump worked for ten minutes as before. The quickſilver in the ſhort barometer-gage fell now nearly to within one-twentieth or five hundredth parts of an inch of that in the ciſtern, and the quickſilver in the long gage roſe nearly to within a twentieth or five hundredth parts of an inch of the height it was at when it was made as a common barometer.

Gages made with theſe precautions ſeem to me to be the moſt to be depended upon, in determining the actual diminution of the preſſure on the ſurface of the quickſilver in the tube of the long gage, and alſo on the ſurface of the quickſilver in the ciſtern of the ſhort gage. But of theſe two gages the long barometer-gage was chiefly uſed in the following experiments, as being fixed to the pump: however, having now made theſe two gages with as much accuracy as I was maſter of, and finding that they agreed pretty nearly, I proceeded to repeat my firſt experiment.

EXPERIMENT II.

I put the ſhort barometer-gage, and the pear-gage with the glaſs cup having a wooden foot, both together under the receiver, which receiver was placed on a leather ſoaked in oil and tallow on the plate of the pump; the pump was then worked for ten minutes, and the quickſilver was brought down in the ſhort barometer-gage to about one-tenth of an inch of the ſurface of the quickſilver in the ciſtern, and roſe in the long barometer-gage to within one-tenth of an inch of the height of the quickſilver in my ſtandard barometer, which was at that time at thirty inches; by which it appeared, that the preſſure on the ſurface of the quickſilver in the ciſtern, and in the tube of the long barometer-gage, was diminiſhed to about a three-hundredth part: the pear-gage being now puſhed down till its open end was immerſed under the ſurface of the quickſilver in the cup, the air was then let in, and the pump appeared by that gage to have exhauſted all but a ſix thouſandth part of the air; or, in other words, the degree of exhauſtion by this gage appeared to be ſix thouſand times.

Finding ſtill this diſagreement between the pear-gage and the other gages, I tried a variety of experiments; but none of them appeared to me ſatisfactory, till one day in April 1776, ſhewing an experiment with one of theſe pumps to the honourable HENRY CAVENDISH, Mr. SMEATON, and ſeveral other gentlemen of the Royal Society, when the two gages differed ſome thouſand times from one another, Mr. CAVENDISH accounted for it in the following manner. "It appeared," he ſaid, "from ſome experiments of his father's, Lord CHARLES "CAVENDISH, that water, whenever the preſſure of the "atmoſphere on it is diminiſhed to a certain degree, is "immediately turned into vapour, and is as immediately "turned back again into water on reſtoring the preſſure. This degree of preſſure is different according to "the heat of the water: when the heat is 72° of FAH-

" RENHEIT's ſcale, it turns into vapour as ſoon as the
" preſſure is no greater than that of three quarters of an
" inch of quickſilver, or about one-fortieth of the uſual
" preſſure of the atmoſphere; but when the heat is only
" 41°, the preſſure muſt be reduced to that of a quarter
" of an inch of quickſilver before the water turns into
" vapour. It is true, that water expoſed to the open air
" will evaporate at any heat, and with any preſſure of
" the atmoſphere; but that evaporation is intirely owing
" to the action of the air upon it: whereas the evapora-
" tion here ſpoken of is performed without any aſſiſtance
" from the air. Hence it follows, that when the receiver
" is exhauſted to the above-mentioned degree, the moiſ-
" ture adhering to the different parts of the machine
" will turn into vapour and ſupply the place of the air,
" which is continually drawn away by the working of
" the pump, ſo that the fluid in the pear-gage, as well as
" that in the receiver, will conſiſt in good meaſure of va-
" pour. Now letting the air into the receiver, all the
" vapour within the pear-gage will be reduced to water,
" and only the real air will remain uncondenſed; conſe-
" quently the pear-gage ſhews only how much real air
" is left in the receiver, and not how much the preſſure
" or ſpring of the included fluid is diminiſhed, whereas
" the

" the common gages ſhew how much the preſſure of " the included fluid is diminiſhed, and that equally, " whether it conſiſt of air or of vapour."

Mr. CAVENDISH having explained ſo ſatisfactorily the cauſe of the diſagreement between the two gages, I conſidered, that, if I were to avoid moiſture as much as poſſible, the two gages ſhould nearly agree: this induced me to make the following experiment.

EXPERIMENT III.

The plate of the pump being made as clean and as dry as poſſible, there was then put on it the before-mentioned ſhort barometer-gage, alſo the pear-gage with a ciſtern entirely of glaſs which held the quickſilver; they were then covered with a receiver, round the outſide of which was laid a cement which perfectly excluded the outward air; every part, before it was put under the receiver, as well as the receiver itſelf, being made as clean and as free from moiſture as poſſible(a). The pump was then worked for ten minutes, and the barometer-gages indicated a degree of exhauſtion nearly 600; the air was then let into the receiver, the pear-gage indicated a degree of exhauſtion, but very little more than 600 alſo. The near agreement of the pear-gage with the barometer-gages in this laſt experiment, in which I had been ſo careful to exclude the moiſture as much as poſſible, ſeemed to prove beyond a doubt, that their diſagreeing in the firſt and ſecond experiments muſt have been owing (as Mr. CAVENDISH ſuppoſed) to the moiſture which in them had not been ſo carefully excluded. But I began now to ſuſpect alſo, that there might ariſe a vapour from ſome moiſture that might be contained in the leather ſoaked in oil and tallow, or in the wooden foot which was cemented to the glaſs cup, both uſed in the firſt and ſecond experiments: theſe ſuſpicions induced me to try the following experiments.

(a) It may be proper here to take notice, that the pump in every experiment hereafter mentioned was worked ten minutes, and the ſame receiver continued cemented to the pump-plate, except where it is otherwiſe mentioned. The top part of this receiver was made to open, in order to put in different things.

EXPERIMENT IV.

A piece of leather dreſſed in allum, known by the name of white ſheep-ſkin, of about four inches diameter, which had been ſoaked in oil and tallow about a year ago (ſuch as was uſed to place the receiver on in the firſt and ſecond experiments) was put into the receiver; the

pump

pump was then worked, and the barometer-gage indicated a degree of exhauſtion of nearly 300; but on the admiſſion of the air the pear-gage indicated a degree of exhauſtion of 4000.

EXPERIMENT V.

The piece of leather being taken out, the pump was then worked, and the degree of exhauſtion appeared by both the barometer and pear-gages to be about 600, as in the third experiment.

EXPERIMENT VI.

A cylinder made of a piece of box wood (which I had kept by me for more than a year) one inch in diameter and three inches in length, was put into the receiver (this piece of wood was of the ſame kind as that which was cemented to the foot of the glaſs cup uſed in the firſt and ſecond experiments) the pump was then worked, and the degree of exhauſtion appeared by the barometer-gage to be 300, but by the pear-gage 16,000.

Theſe experiments have often been repeated, but the reſult was ſeldom the ſame. When leather ſoaked in oil and tallow has been put into the receiver, the pear-gage has ſometimes indicated a degree of exhauſtion of

20,000,

20,000, and ſometimes no more than 500; it likewiſe differs very much with the box wood, which may perhaps be owing to different degrees of heat and moiſture.

From theſe experiments it is evident, that there ariſes an elaſtic vapour from the leather dreſſed in allum and ſoaked in oil and tallow, and alſo from the piece of box wood, when the weight of the atmoſphere has been partly taken off by the action of the pump; and that this vapour preſſes upon the ſurface of the quickſilver in the tube of the long barometer-gage, and of that in the ciſtern of the ſhort one; and that, conſequently, the teſtimony of both theſe gages muſt be influenced by this vapour, as well as by the ſmall remainder of common air: but as it is the nature of the pear-gage not to give its teſtimony till the remaining air contained in it is preſſed, ſo as to become of the ſame denſity of the atmoſphere; and as this vapour cannot ſubſiſt in the form of vapour under that preſſure, this gage is not at all influenced by it, but indicates the remaining quantity of permanent air only.

Seeing thus what a conſiderable quantity of vapour aroſe from the compound of leather, allum, oil, and tallow, my next object was to find out from which of thoſe ſubſtances it chiefly aroſe; how far I have ſucceeded will appear by the following experiments.

Subſtances put into the receiver.	Weight when put into the receiver.	Degree of exhauſtion according to	
		Barom. gage.	Pear-gage.
EXP. VII. Tallow, — —	2 ounces	431	600
EXP. VIII. Oil, — —	2 ounces	377	480
EXP. IX. Allum, — —	2 ounces	378	580
EXP. X. A piece of leather as it came from the leather-ſellers, — — —	100 grains	152	100,000
EXP. XI. The ſame piece of leather ſoaked in the ſame two ounces of tallow and oil melted together, — —		432	800

From theſe experiments it appears, that the elaſtic vapour which cauſed ſo great a difference in the teſtimony of the gages, aroſe principally from the leather, and but little from the tallow, oil, or allum: it even appears by the tenth experiment, that it came from the leather, and ſupplied the place of the exhauſted air ſo faſt, that I could not (at leaſt in the ten minutes) make the barometer-gage indicate a degree of exhauſtion of of more than 159.

To determine whether it was the moiſture in the leather from which the vapour aroſe, I made the following experiments.

Subſtances put into the receiver.	Weight when put into the receiver.	Degrees of exhauſtion according to		Variation in weight during the experiments.
		Barom. gage.	Pear-gage.	
EXP. XII. A piece of white leather, freſh from the leather-ſellers, — —	100 grains	134	100,000	loſt 2 grains.
EXP. XIII. The ſame piece of leather, dried by the fire till it would loſe no more of its weight, — —	80 grains	268	280	gained 2 grs.
EXP. XIV. The ſame piece of leather held in the ſteam of hot water till it had regained the 20 grains it had been deprived of, — —	100 grains	147	100,000	loſt 2 grains.

In this laſt experiment it was full three quarters of an hour before the leather regained the twenty grains of weight, although it was held very near the ſurface of the hot water.

Whenever I have aſſerted, that the degree of exhauſtion, according to the pear-gage, was ſo great as 100,000, I only gueſſed it to be thereabouts, for my gage is not graduated to more than 4000; but, that it may be ſeen what reaſon I had to ſuppoſe it at 100,000, I have brought my pear-gage filled in this laſt experiment, for the inſpection of the gentlemen preſent.

EXPERIMENT XV.

The ſame piece of leather uſed in the eleventh experiment was put into a damp cellar, where it was left till the next day; it was then put again into the receiver, and the degree of exhauſtion, according to the barometer-gage, was 300, and according to the pear-gage 3500.

Being now perfectly ſatisfied that the variation in the teſtimony of the pear and barometer-gages was occaſioned by the moiſture contained in the ſubſtances I had put into the receiver aſſuming the form of vapour; I determined next to try what would be the effect of the vapour which might ariſe from ſmall quantities of different fluids, and from ſome other ſubſtances containing moiſture of various kinds.

Subſtances put into the receiver.	Weight when put in.	Degree of exhauſting according to		Change in weight during the experiment.
		Barom. gage.	Pear-gage.	
EXP. XVI. Water in a watch-glaſs,	3 grains	148	24,000	loſt 1½ grain.
EXP. XVII. Water in a glaſs cup, diameter two inches, —	100 grains	89	8000	loſt 2 grains.
EXP. XVIII. Spirit of wine in the ſame cup, —	100 grains	54	6000	loſt 9 grains.
EXP. XIX. Vitriolic acid, —	100 grains	340	220	gained 1 gr.
EXP. XX. A piece of the inſide of a china orange with ſome of the rind, —	100 grains	160	100,000	loſt 2½ grs.
EXP. XXI. A piece of the inſide of an onion, —	100 grains	160	100,000	loſt 1½ grain.
EXP. XXII. A piece of tainted beef,	100 grains	152	100,000	loſt 2½ grs.
EXP. XXIII. A piece of freſh beef,	100 grains	136	100,000	loſt 2½ grs.
EXP. XXIV. Spirit of turpentine,	100 grains	301	1800	loſt 2 grains.
EXP. XXV. Pearl-aſh, —	2 ounces.	118	5000	
EXP. XXVI. The ſame pearl-aſh made very hot, —		198	420	
EXP. XXVII. A lighted candle held in the receiver till it went out, — —		297	1800	

Subſtances put into the receiver.	Weight when put in.	Degrees of exhauſting according to		Change in weight during the experiment.
		Barom. gage.	Pear-gage.	
EXP. XXVIII. A piece of charcoal,		129	1800	
EXP. XXIX. The receiver heated by holding ſeveral pieces of lighted charcoal in it, and then the above piece being thoroughly lighted was put into the receiver, and the pump worked, —		650	600	
EXP. XXX. Camphire, —	100 grains	304	520	loſt barely ½ a grain.
EXP. XXXI. Sulphur made to burn on a piece of braſs, —		247	320	

Obſerving by theſe experiments that the ſmall quantity of moiſture which exhaled from the ſubſtances under the receiver prevented the pump from exhauſting it to any very conſiderable degree, I began to ſuſpect that whenever wet leather had been uſed to connect the receiver with the plate, there muſt have riſen ſo great a quantity of vapour as to have prevented the degree of exhauſtion from being near ſo great as in ſome of the foregoing inſtances. Theſe ſuſpicions induced me to make the following experiments.

	Degrees of exhauſtion according to	
	Barom. gage.	Pear-gage.
EXP. XXXII. The receiver was taken off, and after the cement was wiped clean from it, and every part made perfectly dry, it was put again on the pump-plate, and a little oil only was poured round the outſide edge,	nearly 600	full 600
EXP. XXXIII. The receiver was taken off again, and inſtead of the oil it was ſet on a piece of leather, which had been ſoaked two days in water, —	51	16,000
EXP. XXXIV. The laſt experiment repeated with the ſame piece of leather, — —	51	1500
EXP. XXXV. The laſt experiment repeated again with the ſame piece of leather, — —	51	1000
EXP. XXXVI. The receiver was taken off, and inſtead of the leather ſoaked in water, there was put on a piece of the ſame ſort of leather, ſoaked in a mixture of water and ſpirit of wine, ſuch as Mr. SMEATON uſed,	47	12,000
EXP. XXXVII. The laſt experiment repeated with the ſame leather, — — —	47	1150
EXP. XXXVIII. The laſt experiment repeated again with the ſame leather, — —	47	500

The great difference in the teſtimony of the pear-gage in theſe ſix laſt experiments appeared to me exceedingly aſtoniſhing, for the leathers ſeemed each of them to be as moiſt at laſt as at firſt.

By theſe experiments I was convinced how effectually the uſe of leather ſoaked in water, or in water and ſpirit of wine, prevents the pump from exhauſting to any conſiderable degree. I have made a number of experiments of the ſame kind as theſe; but have never been able to exhauſt, under ſuch circumſtances, to a greater degree than between 50 and 60, when the heat of the room was about 57° by a thermometer of FAHRENHEIT's ſcale: but the following experiments will ſhew how much ſome different degrees of heat affect the degree of exhauſtion.

	Height of the Therm.	Degrees of exhauſtion according to	
		Barom. gage.	Pear-gage.
EXP. XXXIX. Receiver ſet on leather which had lain all night in water, —	46	84	20,000
EXP. XL. Receiver ſet on a leather ſoaked all night in two parts water and one of ſpirit of wine, — — —	46	76	8000

The pump having been put in a room of the heat of 57° of FAHRENHEIT's ſcale for ſeven hours together, with the leathers put in the ſame water and the ſame ſpirit of wine and water which they had been ſoaked in all night, and which had been uſed in the two laſt experiments, the following experiments were made.

	Height of the Therm.	Degree of exhauſtion according to	
		Barom. gage.	Pear-gage.
EXP. XLI. The receiver ſet on the leather ſoaked in water, — —	57	56	16,000
EXP. XLII. Receiver placed on a leather ſoaked in water and ſpirit of wine, —	57	49	1200

The following table will ſhew the comparative excellency between the pump on Mr. SMEATON's principle with which the chief of theſe experiments have been tried, and one of my common double barreled table air-pumps under the ſame circumſtances. The leather on the piſtons of both was ſoaked in oil and tallow, and the receiver cemented down to each plate; the pumps were both of them freſh oiled.

	Pump on Mr. SMEATON's principle. Degrees of exhauſtion according to		Common pump. Degrees of exhauſtion according to	
	Barom. gage.	Pear-gage.	Barom. gage.	Pear-gage.
EXP. XLIII. A piece of leather, weighing 100 grains, as it came from the leather-ſellers, was put into the receiver of each pump, both pieces being cut from the ſame ſkin cloſe by each other, —	152	100,000	108	12,000
EXP. XLIV. The ſame pieces of leather dried by the fire till they would loſe no more of their weight, — —	506	520	160	165

The following experiments will ſhew the effect of water uſed in the barrels of pumps to make the piſtons move air tight in them.

I took the ſame common air-pump uſed in the laſt experiment, and having taken off the leathers ſoaked in oil and tallow from the piſtons of this pump, and wiped the barrels as clean as poſſible, I then put new leathers, which had been ſoaked in water, and new bladder valves; the receiver was then cemented to the pump-plate as before.

	Degrees of exhauſtion according to	
	Barom gage.	Pear-gage.
EXP. XLV. The pump was then worked as uſual, —	37	38
EXP. XLVI. The laſt experiment repeated with another common pump, the leathers of the piſtons of which were alſo ſoaked in water, — —	34	37

From theſe experiments it evidently appears, that the air-pump of OTTO GUERICKE, and thoſe contrived by Mr. GRATORIX, and Dr. HOOKE, and the improved one by Mr. PAPPIN, both uſed by Mr. BOYLE, alſo HAUKSBEE'S, S'GRAVESANDE'S, MUCHENBROOK'S, and thoſe of all who have uſed water in the barrels of their pumps, could never have exhauſted to more than between 40 and 50, if the heat of the place was about 57; and although Mr. SMEATON, with his pump, where no water was in the barrel, but where leather ſoaked in a mixture of water and ſpirit of wine was uſed to ſet the receiver on the pump-plate, may have exhauſted all but a thouſandth or even a ten thouſandth part of the common air, according to the teſtimony of his pear-gage; yet ſo much vapour muſt have ariſen from the wet leather, that the contents of the receiver could never be leſs than a ſeventieth or eightieth part of the denſity of the atmoſphere: nevertheleſs, it does not ſeem that any deficiency in the conſtruction of Mr. SMEATON'S pump was the cauſe of his not being able to exhauſt beyond the low degrees of 70 or 80. Had he been aware of the bad effects of ſetting the receiver upon leather ſoaked in water and ſpirit of wine; and had he made uſe of the precaution to free all parts of his pump as much as poſſible from moiſture, I make not the leaſt doubt but the air-pump, which he executed himſelf, would have exhauſted to as great a degree, as that pump has been ſeen to have done with which the chief of theſe experiments were made.

Having read the principal part of this paper to Mr. SMEATON, and ſhewn him ſome of the experiments; one in particular, where the pear-gage, as he obſerved himſelf, was filled to no leſs than 100,000th part of the whole content; he remarked from memory, that he had in ſeveral trials exceeded 1000 times, and once, as he remembered, near or about 10,000 times; but as he never could account how this happened, which appeared to him perfectly accidental, and therefore could not depend upon doing it at pleaſure, he contented himſelf with putting down 1000 times, as being what (under the circumſtances mentioned in his papers) he had a tolerable certainty of.

I muſt here again obſerve, that if we only wiſh to know the quantity of permanent air remaining in the receiver after it is as much exhauſted as poſſible,

ble, it ſeems, that it is by Mr. SMEATON's gage only that we can know it. Again, when by the aſſiſtance of his gage and the barometer-gage together, we have diſcovered that there is a vapour which ariſes and occupies the place of the permanent air which is exhauſted, it ſeems that it is by the means of his gage only that we can diſcover what part of the remaining contents of the receiver conſiſts of this vapour, and what part of permanent air.

An account of ſome further experiments made with the ſame air-pump on Mr. SMEATON'S *principle, the reſults of which were different from the former.*

AFTER I had made the foregoing experiments, and thought to have done with the ſubject (for ſome time at leaſt) in reviewing them for the laſt time, I perceived one or two, the extraordinary reſults of which (though not unnoticed by me before) I now thought I had not paid ſufficient attention to. Experiment the 19th (in which I found that when vitriolic acid was put into the receiver, and the pump worked for the uſual time, the pear-gage indicated a much leſs degree of exhauſtion than the barometer-gage) ſeemed to me now ſo ſurprizing, that it was impoſſible not to wiſh to repeat this experiment with all the care poſſible, and to endeavour to recollect all the circumſtances which I thought could any way influence the reſult.

The vitriolic acid I made uſe of in the following experiments was ſome that I had had by me for ſome time; it had been kept in a phial, ſtopped with a glaſs-ſtopper, and tied over with a bladder. The thermometer in the room was at 59°, and the weather remarkably dry.

	Weight when put into the receiver.	Degrees of exhauſtion according to		Variation of weight during the experiment.
		Barom. gage.	Pear-gage.	
EXP. XLVII. Vitriolic acid in a glaſs cup, two inches diameter,	100 grains	602	380	gained 1 gr.
EXP. XLVIII. The laſt experiment repeated with the ſame vitriolic acid in the ſame cup,	101 grains	502	350	gained ½ a gr.
EXP. XLIX. The former experiment repeated again with the ſame vitriolic acid in the ſame cup, — —	101½ grs.	502	350	
EXP. L. The former experiment repeated the fourth time in every reſpect as before,	101½ grs.	502	340+	gained a ¼ of a grain.

The generation of vapour in the exhauſted receiver which Mr. CAVENDISH had ſuppoſed, and which I ſeemed, by my former experiments, to have proved, appeared ſatisfactorily to have accounted for the pear-gage's indicating a greater degree of exhauſtion than the barometer-gage: but what to ſuppoſe could poſſibly make it indicate a leſs, I was entirely at a loſs; for after having made theſe ſurprizing experiments on the vitriolic acid, I wiſhed once more to try if I could repeat, with the ſame reſult, ſome of my former experiments, in which the pear-gage had indicated ſo nearly the ſame degree of exhauſtion as the barometer-gages. Accordingly I took away the vitriolic acid from under the receiver; the pump was then freſh oiled, and I was very careful to wipe clean and dry the receiver and pump-plate, and then cemented down the receiver as uſual.

EXPERIMENT LI.

The pump was then worked, and the degree of exhauſtion appeared by the barometer-gage to be 432; but by the pear-gage to be but 370.

I know of no circumſtance attending this experiment that differed from thoſe in which my former experiments were made when the gages agreed ſo nearly, unleſs it were that of the weather: I recollect that it was then very damp, and now it had been very dry for ſome time. How this circumſtance could make ſo great an alteration in the reſult of theſe experiments, I cannot pretend to ſay; but ſome of the following experiments will ſhew that the pear-gage ſtill continued in many caſes to indicate a leſs degree of exhauſtion than the barometer-gage.

In all the preceding experiments the pump was worked for ten minutes, and the pear-gage was at the end of that time puſhed down ſo as for the mouth to be immerſed in the ciſtern of quickſilver, and the air then let in according to the manner of uſing this gage; but now that I found that the teſtimony of this gage ſo ſeldom agreed with that of the barometer-gage, I wiſhed to try, whether they might not agree when the receiver was exhauſted only in part, though they did not when it was exhauſted as much as poſſible. For this purpoſe I had the ſame receiver fitted with two pear-gages, ſo that I now could immerſe the mouth of one of them in the ciſtern of quickſilver when I had exhauſted the receiver in part only, and not immerge the mouth of the other till the receiver was exhauſted for the uſual time of ten minutes; in which time I found I could always raiſe the quickſilver in the barometer-gage as high as if I were to work the pump much longer.

	Degrees of exhauſtion when the pump had been worked			
	Five minutes; according to		Ten minutes; according to	
	Barom. gage.	Pear-gage.	Barom. gage.	Pear-gage.
Exp. LII. The receiver cemented to the pump-plate, —	430	300	430	360
Exp. LIII. The receiver uncemented, wiped clean from the cement, and put on the pump-plate, with a little oil round the outſide, —	502	360	502	360
Exp. LIV. The receiver put on a leather ſoaked in oil and tallow,	502	320	323	500

This laſt experiment ſeemed very extraordinary; for after having worked the pump for five minutes only, the barometer-gage indicated a degree of exhauſtion of 502; but by working the pump five minutes more, it indicated a leſs degree of exhauſtion, *viz.* 323. This effect I have obſerved to happen more than once.

	Degrees of exhauſtion when the pump had been worked			
	Five minutes; according to		Ten minutes; according to	
	Barom. gage.	Pear-gage.	Barom. gage.	Pear-gage.
Exp. LV. Receiver put on a leather ſoaked in water for one night,	47	380	63	8000
Exp. LVI. Receiver put on a leather ſoaked for one night in a mixture of two parts water and one ſpirit of wine, — —	48	300	50	1200

The receiver was then taken off from the pump-plate, and a blank ſcrew ſcrewed into the hole in the pump-plate.

EXPERIMENT LVII.

The pump was then worked for ten minutes without any receiver on the pump-plate. The barometer-gage alone being afterwards connected with it, the gage indicated a degree of exhauſtion of 50 only, which was the ſame as in the laſt experiment; ſo that the bad effects of the mixture of the ſpirit of wine and water ſtill continued.

I then poured about two ſpoonfuls of oil down the hole in the pump-plate, and the piſtons were worked gently till moſt of the oil had paſſed through the pump into a reſervoir made to receive it. A blank ſcrew was then ſcrewed into the hole in the pump-plate as before, and after the pump had been worked for a minute or two, four or five times, the air being let in between each time, the oil had then waſhed ſo much of the moiſture out of the inſide of the pump, that I was now able to exhauſt to 430 inſtead of only 50 times, as before the oil had been made to paſs through it.

During the courſe of theſe experiments on the air-pump it appeared, by the teſtimony of the pear and barometer-gages, that the remaining contents of a receiver, when exhauſted as much as poſſible, was at different times of different kinds; ſometimes it ſeemed to conſiſt entirely of permanent air, as when a little vitriolic acid, &c. was put in the receiver; and ſometimes moſtly of vapour ariſing from moiſture, and but a very ſmall proportion of permanent air, as when a bit of damp leather, &c. was in the receiver. I was now therefore deſirous of ſeeing what appearance the electric matter would exhibit in theſe different rarified media.

For this purpoſe I had a glaſs tube made, of an inch bore, and four feet and a half in length. This tube was connected to the receiver of the air-pump by means of an elbow-piece of braſs, to which it was cemented; which elbow-piece was inſerted perpendicularly in the top of the receiver: as the elbow made a right angle, the tube itſelf was of courſe horizontal.

Moreover, at that end of the glaſs tube which was cemented to the braſs elbow-piece, there was fixed, on the inſide, a piece of braſs wire, about three inches and a half long, filed to a point, and pointing towards the other end of the tube. At the other end of the tube was cemented a braſs ſcrew fitted to a braſs cap, which ſcrewed on it; and in this braſs cap was fixed a braſs wire, three inches and a half long, which pointed towards

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the braſs point; at the other end of the tube, and at the end of this wire, was a braſs ball, eight-tenths of an inch diameter.

The braſs cap at this extremity of the tube which is fartheſt from the receiver was made round, and placed ſo as to be in contact with the prime conductor of an electrical machine.

I now firſt put ſome vitriolic acid into the receiver, as a means of being able to make the remaining contents of the receiver, when exhauſted as much as poſſible, to conſiſt of permanent air only, unadulterated with vapour; and as the receiver was the ſame I had uſed in my foregoing experiments, there were two pear-gages fitted to it, which pear-gages I puſhed down into the ciſterns of quickſilver at different times, and the pump was worked as in my former experiments for ten minutes: heat of the room 59°.

The electrical machine was worked during the whole of the experiments.

EXPERIMENT LVIII.

Electrical appearances exhibited.	Degrees of exhauſtion according to	
	Barom. gage.	Pear-gage.
Light began firſt to appear in flaſhes, — —	5	
Light appeared the whole length of the tube in ſtriæ,	8	
Tube was filled with an uniform body of pale light,	74	75
The pump had now been worked five minutes.		
The pump was then worked five minutes more.		
The tube was ſtill filled with a uniform body of pale light, — — — —	269	230

The conductor being then removed to a diſtance from the tube, it was made to approach it by degrees till a ſpark ſtruck it, which was at the diſtance of two inches; the light in the tube now appeared like a compact body of fire, of a vivid purple colour, tending to a red.

Objects were ſeen through the tube when filled with this body of the electrical light, no leſs diſtinctly than if there had been no ſuch light in the tube(a).

The vitriolic acid being taken out of the receiver, I put a piece of leather of 100 grains, as it came from the leather-ſellers, into that end of the tube which was next to the conductor of the electrical machine, and fartheſt from the receiver of the air-pump. I put the leather in at this end of the tube rather than into the receiver, to be ſure that the tube might be filled with the vapour ariſing from the damp leather rather than with the common air. The pump and electrical machine were then worked as before.

(a) This circumſtance has been before remarked by Dr. HAMILTON in his conjectures on the tails of comets.

EXPERIMENT LIX.

Electrical appearances.	Degrees of exhauſtion according to	
	Barom. gage.	Pear-gage.
Light began firſt to appear in flaſhes, — —	12	
Light appeared the whole length of the tube in ſtriæ,	22	
Light vaniſhed ſcarce to be ſeen, — — —	90	100
The pump had now been worked ſeven minutes. The conductor was now removed from the tube, and the greateſt ſtriking diſtance was found to be one inch,		
The tube now appeared luminous, but the light was faint and white. The conductor was then again put in contact with the tube, and the machine worked: the pump was alſo worked for three minutes more, but ſcarce any light appeared, — —	148	20,000

The conductor was then again removed from the tube, and the ſtriking diſtance was found to be only one inch and four-tenths: the tube at the time of ſtriking was luminous as before, and the light was of the ſame faint white colour.

Having lately received from my friend Dr. LIND ſome æther prepared by the ingenious Mr. WOLFE, I was very deſirous to try whether I could produce any conſiderable degree of cold by the evaporation of æther under a receiver whilſt exhauſting. For this purpoſe I put the æther into a phial, the neck of which was ſufficient to admit the ball of a thermometer: this being placed on the air-pump, under a receiver which had a plate at the top, with a wire paſſing through a collar of leathers; to this wire the thermometer was fixed, by which means I could eaſily dip the ball of the thermometer into the æther.

EXPERIMENT LX.

The pump was now worked, and whilſt the receiver was exhauſting, the ball of the thermometer was often dipped into the æther; and when the degree of exhauſtion by the barometer-gage was 65 (which was the utmoſt in this caſe that the pump would exhauſt to) the degree of cold indicated by the fall of the quickſilver in the thermometer was 48° below 0 on FAHRENHEIT'S ſcale; ſo that there was a degree of cold produced 103° colder than the air in the room where the experiment was made, the thermometer in it being at 55° degrees above 0. The pump was kept continually working for half an hour, and the ball of the thermometer often dipped into the æther; but no greater degree of exhauſtion or cold could be produced. The air being let into the receiver, the quickſilver in the thermometer roſe 10°, *viz.* to 38° below 0.

EXPERIMENT LXI.

Freſh æther being put into the phial to what was remaining, the thermometer roſe to 30° above 0: the pump was then worked again conſtantly for half an hour; yet by the barometer-gage the degree of exhauſtion was now not more than 16, and the degree of cold produced did not exceed the 11th degree below 0, as appeared by the quickſilver in the thermometer. The air being let into the receiver, the remaining æther was examined, and there were found ſeveral pieces of ice at the bottom of the phial, ſome of them as big as large peas, which, when the æther became nearly of the heat of 32° or freezing point of water, were intirely diſſolved.

The air-pump with which theſe experiments were made exhauſted above 400 times before the æther was put under the receiver.